

40
P. 7689
9
21 8 10
SOME PROBLEMS

IN

TROPICAL EPIDEMIOLOGY.

AN ADDRESS

DELIVERED TO

THE EPIDEMIOLOGICAL SOCIETY OF LONDON

BY

DR. PATRICK MANSON, C.M.G., F.R.S., 154-1922

PRESIDENT OF THE SOCIETY,

Medical Adviser to H.M.'s Colonial Office.

28 MAY 1922

LONDON:

PRINTED AT THE BEDFORD PRESS, 20 & 21, BEDFORDBURY, W.C.

1901.

It will be gratifying to the Society, as well as to the Profession in general, to learn that since this Address was delivered a gentleman (who does not wish his name disclosed) has promised £500 towards the £2000 required for the projected expedition to the South Pacific.



11, CHANDOS STREET,

CAVENDISH SQUARE, W.,

January, 1901.

This Presidential Address is printed and circulated in accordance with the following Resolution, which was submitted by Dr. Bond and Dr. Franklin Parsons (Ex-President of the Society), and carried unanimously.

“RESOLVED, that Dr. Patrick Manson’s Presidential Address be forthwith published and circulated, and that copies thereof be forwarded to the Foreign and Colonial Offices, to the Local Government Board, and to the more important Corporations and City Companies.”

“RESOLVED further, that the Society expresses its full concurrence with the views set forth in the Address as to the desirability of despatching a scientific expedition to the Pacific Islands, there to study the outstanding problems of malaria. The Society joins with its President in expressing a hope that some Government Department, Society, Corporation, Company, or Individual, may be willing to furnish wholly, or in part, the necessary funds, which are estimated at £2000.”

H. TIMBRELL BULSTRODE, M.A., M.D. } *Honorary*
J. W. WASHBOURN, M.D., F.R.C.P. } *Secretaries.*

Further information with regard to the scheme may be obtained from the Honorary Secretaries at the above address.



INAUGURAL ADDRESS OF SESSION 1900-1901.

SOME PROBLEMS IN TROPICAL EPIDEMIOLOGY.

BY DR. PATRICK MANSON, C.M.G., F.R.S., ETC., PRESIDENT.

(Read November 16th, 1900.)

My first duty in taking the Chair is to thank you, gentlemen, for the honour you have done me in electing me President of this important Society. I set a high value on the compliment. At the same time, I feel that I have very small claim to regard the honour as personal to myself. I feel that my election is intended as an acknowledgment that the branch of medicine to which I have devoted much of my energies—Tropical Medicine—is at last taking its proper place in the estimation of the profession. In the past there have been many reasons why the claims of this department of practice were inadequately recognised. It is all the more gratifying to feel that better days are in store for it.

In any body of scientific men, particularly in a catholic profession like that of medicine, and still more in such a Society as this, it is not meet that the purview be too circumscribed—too provincial. The mental horizon of the true scientific spirit is determined neither by size, distance, nor social conditions. To the scientific mind the infinitely minute bacterium is just as important as the gigantic elephant, a thing thousands of miles away as important as one close at hand, a bird or a beast as a man, a naked savage as a European potentate. And so should it be as regards disease. From a scientific standpoint, the ailments of the native, it may be of the European in some distant land, are just as important and worthy of scientific study as are those of the stay-at-home Englishman. Theoretically, this has been recognised since medicine first pretended to be scientific; practically, its recognition is of recent date.

But although this recognition is of recent date, the concession, if I may so designate it, has been fully justified,

for, within the last decade or two, in no department of medicine has progress been more conspicuous; and not only has the subject itself advanced by leaps and bounds, but in its progress it has thrown important light on pathological and epidemiological problems in connection with the diseases of temperate climates.

In quite recent years tropical pathology, and I would join to it comparative pathology, have begun to exercise a powerful influence on general pathology; they have supplied the latter with new facts, and with what is sometimes as important as facts, new ideas. They have shown that in their diseases man and beast have much in common; they have demonstrated the hitherto almost unsuspected fact that insects, and other organisms far lower in the scale, are important, and, in some instances, essential agents in the diffusion of disease; and, not the least of the benefits they are conferring on medicine, by supplying instances of disease germs other than bacteria they are exercising a restraining influence on the too-exclusive worship of the bacterium. Finally, they have shown that there are other ways of acquiring disease than through meat and drink.

Of the many lessons we have received from the recent tropical work, two stand out with special distinctness. One appeals to the pathologist, the other to the epidemiologist. The first is, that we have now to include the protozoa and the nematodes among the important pathogenetic organisms affecting man; the second, that many diseases are spread in a variety of ways by blood-sucking arthropods. To what length these newly-apprehended facts will carry us in pathology and epidemiology it is difficult to foresee, but of late each month brings us fresh evidence that they must be reckoned among the cardinal principles in medicine.

The groups of diseases associated with the various malarial parasites, and those associated with the various blood-worms, and the relation of their respective organisms to the mosquito, constitute as yet the best and most complete, though by no means the only, illustration of these new principles. The relation of the parasite to the disease, and the relation of the insect to the parasite, and thereby to the disease, have been thoroughly established. It remains only to work out the detail. When I say *only*, I do not mean to imply that little remains to be done. On the contrary, the work entailed by, and springing out of, Ross's great discovery that the mosquito

is the active agent in the diffusion of malaria, and of the discovery that the same insect is the active agent in the diffusion of filariasis, can best be described as *immense*. In this, as with the discovery of every new principle, the first step forward serves but to reveal the vastness of the field that has been entered. When we begin to think of, and to work at, this matter of the mosquito and disease, problem after problem crops up, and many of them on humanitarian, not to mention scientific, grounds urgently call for immediate solution.

For example, we already know that certain species of mosquito are efficient definitive hosts of the malarial parasites, and we also know that other species are not efficient; but, so far, we are not acquainted with all the efficient species, nor with all the inefficient species. We already know one species of mosquito which is an efficient intermediate host for *Filaria nocturna*, but we know little or nothing as to the efficiency, or the reverse, of other species. Besides *Filaria nocturna*, there are at least four other blood-worms which affect the tropical man. As yet we do not know what are their intermediate hosts. We do not even know if these intermediate hosts are mosquitoes. Considering that there are, according to Giles, two hundred and seventy species of mosquito known to zoologists, and probably nearly as many more which are as yet unknown or undescribed, each of which has to be tested as regards its relationship to the various malarial parasites and the various blood-worms, it is evident that in this matter alone there lies a prodigious task for the investigator, one demanding years of labour and many workers.

Again, we know that the malarial parasite differs in virulence in different localities, and this notwithstanding that the morphological characters of the parasite in these different localities are identical. Does this difference in virulence, and what I might call strain, depend on the passing of the parasite through different species of mosquito; on the difference of the culture media, so to speak? Assuming as probable, although not proved, that the deadly Blackwater Fever of Africa is produced by the malarial parasite, the question forces itself upon us: Does this special virulence result from the passage of the parasite through a special mosquito, *Anopheles funestus*, for example, a mosquito widely diffused in malarial Africa, and, I understand, general in the Blackwater Fever zone?

Certain countries with apparently suitable climates, and offering suitable hydraulic conditions, are nevertheless free

from malaria and free from filariasis. Does this immunity depend solely on the absence in these countries of appropriate species of mosquito? Certain countries are malarial, but are free from filarial disease. Does this depend on the presence of malarial mosquitoes, and the absence of filarial mosquitoes? Other countries are afflicted with filarial disease, but have no malaria. Does this depend on the presence of the filarial mosquito and the absence of the malarial mosquito? Finally, in some countries we find malarial and filarial disease flourishing side by side. In such countries both kinds of mosquito must be present. How are these things to be explained?

These are but a sample of the many problems in tropical disease which at the present moment cry for solution. Their solution demands enormous and earnest labour.

The leading facts in science have oftentimes been discovered, as it were, by accident. Later, after a certain amount of elaboration, there comes a time when, in consequence of growing complexity, the subject has to be split up into sections, each of which must be worked separately, perhaps by different individuals. At this more advanced stage, little if any progress can be made by what I may designate promiscuous work. Too often such work—and there is a great deal of it—is, as regards progress, but lost labour, so far as genuine advance of the subject may be concerned, however it may be as regards the education of the individual. The knowledge of the pathological and epidemiological relations of the mosquito have reached this point. The subject has so grown that it has to be split up into sections. Each problem, after thoughtful formulation, must now be carefully worked out by a special investigation. This is the only way to advance, as it is the only way to economise the somewhat limited supply of pathological and epidemiological energy in the market.

I have given some thought to one of the mosquito problems to which I have just alluded, namely, the apparently capricious distribution of malarial disease and of filariasis, the explanation of this, and how to seek for it. When I found I had to give an address here this evening, I thought I might use the opportunity to state my views on this subject, and to submit to your consideration and criticism my conclusions and a method by which, in my opinion, the solution of the problem might be attempted. Let me re-state the problem. Why is malaria present in one country and absent in another country, although both countries possess, to all appearance, the same meteorologi-

eal and hydraulic conditions? Why is the same the case as regards filariasis. And why do we find occasionally the two diseases, malaria and filariasis, sometimes occurring in the same country?

The explanation I offer is this. The presence or absence of malarial disease, or of filariasis, in any particular locality depend on the presence or absence of their respective subserving mosquitoes in sufficient numbers: the presence or absence of the respective mosquitoes being determined, not so much by the presence or absence of the essentials for mosquito life, namely, water, adequate atmospheric temperature and vegetable food, but by the presence or absence of special conditions *inimical* to special kinds of mosquitoes.

My reasons for this conclusion you will gather from my remarks on the method by which I would suggest that their establishment might be profitably attempted. If I am right, such an investigation, successfully conducted, might lead to results as regards the prevention of malaria of the highest practical as well as epidemiological importance. The method I allude to implies an investigation extending over a considerable period, say one or two years. To secure continuity of work, and to minimise risk of interruption by ill-health or other contingencies, it would be better to have two investigators. These I would send, properly equipped, to one of the smaller Pacific Islands, Samoa, for example, where there is no malaria. I say Samoa, because it affords many of the conditions of civilised centres. There are many other islands, however, which might be selected. In one of these islands the investigators would study the local mosquito fauna exhaustively. When they had completed this work, I would send them to another small island in the vicinity where malaria is known to be endemic and plentiful, and there I would set them again to study the local mosquito fauna. Next, I would get them to transport stocks of malarial *anopheles* from this latter island to Samoa or other malaria-free Pacific island, and there to endeavour to breed them, doing this at first under laboratory conditions. Having become thoroughly acquainted with the necessary laboratory conditions, and having accumulated a large stock of *anopheles*, I would set the investigators to place in their aquaria various objects, such as plants or animals, which might be considered as peculiar to Samoa, in the hope that in some of these they would find something inimical to *anopheles*. I would then try to breed the insects under more natural conditions, in artificial or natural puddles, and side by side

with the local *culices*, and study to find out what favoured *anopheles* and what repressed them.

If, as I anticipate, something were thereby discovered which when introduced into the water proved fatal to the experimental *anopheles*, and if this something were absent in the malarial island previously explored, we should be justified in concluding that this something was connected with the absence of *anopheles* in Samoa.

In the same way the investigators would transport the Samoan *culices* to the neighbouring malarial island, where it had already been ascertained that there was no filaria. By the same process of introducing one substance after another into mosquito cultures, the investigators would endeavour to arrive at a circumstance which is inimical to the filaria-nurturing *culex*, and which was not present in Samoa.

Having found the anti-*anopheles* and the anti-*culex* conditions, the investigators would next study these conditions with the object of ascertaining if such could be turned to practical account.

I do not think that it would be found that the distribution of special species of mosquito is directly determined by any vegetable or mineral substance. I think the determining agencies are more likely to be animal or bacterial, very likely of the nature of a disease germ to which in the one case *anopheles* is immune, and in the other case *culex* is immune. We know that insects and their larvæ are exceedingly liable to protozoal invasion. If such a disease germ were found, it would then demand special study. Its favouring conditions would have to be ascertained. The investigation in this way might become very complex and difficult, and, you may think perhaps, interminable; but the more complex the conditions of mosquito life and death, the more likely are we to find a weak link in the chain—a link which we might be able to sever. Difficulty and complexity are no reasons for shirking a problem; in this instance, quite the reverse.

These suggested investigations, as stated, are planned on the assumption that *anopheles* of a malaria-favouring species cannot, owing to some local condition, flourish in malaria-free places. Such is the state of our ignorance about the distribution of the different species of mosquito that, so far as I can ascertain, it is not known to dipterologists what species, if any, of *anopheles* inhabit Samoa and the other malaria-free islands of the Pacific. In view of this ignorance, we must not close our eyes to the possibility

that the absence in these islands of malarial mosquitoes depends, not on the existence of conditions inimical to malarial *anopheles*, but to the circumstance that such mosquitoes are not indigenous to these islands, and that hitherto they have not been introduced. This opens an interesting and important question which it would be well to settle while it is yet possible, and which could easily be settled by such a scheme of investigation as I project. That exotic mosquitoes, like many other insects, can be introduced and become permanently established in a country is most probable. There is some evidence that such has been the case in Australia. Skusé, quoted by Giles, writes: "One species appears to have been introduced into this country (Australia), judging from the accounts of all colonists, and is possibly a variety of *Culex ciliaris* L. It may have been imported from Europe in the tanks of some of the old sailing ships. As the railway extends, so this mosquito reaches portions of the country hitherto exempt from it; and it has been, and is being, communicated to other places along the coast by water traffic."

According to Giles, *Culex ciliaris* is not a European species; and as Bancroft has proved it to be an efficient intermediary for *Filaria nocturna*, a parasite which is not indigenous to Europe, I incline to the opinion that *Culex ciliaris* did not come to Australia from Europe, but from the filaria-haunted South Sea Islands, probably in one of the old whaling ships, or by some similar channel. Now, if *Culex ciliaris* could be so imported into Australia, malaria-fostering *anopheles* might be similarly introduced from Australia, say from Queensland where it exists in plenty, into the South Sea Islands; and then, unless there are local protective conditions, such as I have assumed to exist, these hitherto immune islands would become malarial and unhealthy.

Undoubtedly, malaria germs have frequently been introduced into the South Pacific. Chinese, Indians, and Europeans, with malarial *gametes* in their blood, must often have visited them. Certainly, such individuals have visited Fiji, one of the malaria immune islands, yet the disease has not spread there. Why? Manifestly, because the proper and necessary mosquito host is not among the fauna of these islands.

Although the recent appearance of malaria in Mauritius which until the sixties was absolutely free from indigenous malaria, is a historical fact, we do not know for certain how its introduction was brought about. It is quite certain that malarial fevers occurred among the garrison

and among the visitors to the island, years before the historical epidemic explosion. These, however, were evidently cases in which the infection had been acquired in India, Madagascar, and elsewhere; but although the *gametes* of malaria must thus for many years prior to 1867 have been imported in abundance into the island, the disease did not spread until after the date I mention. Why? Because the necessary mosquito host was not present. But about the date mentioned, although there is no direct evidence of the fact, *anopheles* must have been introduced from without, and gradually spread over the island, subsequently reaching the neighbouring island of Réunion. The march of the epidemic, so carefully and accurately described by Davidson, tallies exactly with such a hypothesis.

Of the many theories which have been promulgated to explain the introduction of malaria into Mauritius, none meets all the circumstances so completely as the one I allude to.

Now, if a malaria-nurturing *anopheles* has been successfully introduced into Mauritius, with consequences so disastrous to that island, may not a similar calamity befall the Pacific Islands? May not the absence of *anopheles* there depend on the circumstance that it has not been introduced from without? For hygienic reasons, therefore, as well as for scientific purposes, would it not be well, before it is too late, to ascertain the exact position at the present day of the mosquito fauna of these islands? If, subsequently, owing to the increasing communications between the Pacific Islands and malarial countries, malaria *anopheles* and with them malaria be introduced, we shall then know the reason, and in consequence of this knowledge be better able to cope with, and perhaps to limit the extension of, what for these islands would be a calamity indeed. This is another reason for instituting the investigation I suggest.

Although it may be found that the freedom from malaria which many of the Pacific Islands still enjoy does not depend on the presence in them of the conditions inimical to the malaria *anopheles*, my contention may still hold good that local immunity from malarial *anopheles* often, perhaps generally, depends on the presence in these localities of something which is directly inimical to the insect, and not on the absence of the essential hydraulic and meteorological conditions.

I have selected the Pacific Islands as the field for the suggested investigation, because these islands are small,

and therefore less likely than larger countries to present complicating conditions. If it be found that the immunity of the Pacific Islands depends solely on the fact that hitherto malarial *anopheles* have not been introduced, investigators can still fall back on what I might term the islands of immunity which are to be found in many malarial countries, even in those which are the most malarial. I selected the Pacific Islands for another reason: they present unique opportunities for the study of the pathology and epidemiology of certain other tropical diseases correlated to the mosquito, especially filariasis, and what I believe to be a filarial disease also, *Elephantiasis arabum*.

It has been taken for granted—somewhat too readily, I think—that the latter disease belongs to the filarial group. I believe it does so belong, but I cannot absolutely prove it. We frequently find that elephantiasis supervenes on what are certainly filarial diseases. Elephantiasis is essentially a disease of the lymphatic system, depending on an obstruction in that system, just as in the case of the filarial diseases. Like them, it is attended by a special kind of lymphangitis and fever, and, so far as known, it corresponds in geographical distribution, frequency, and intensity, with the geographical distribution of filariasis. But although thus associated in those countries in which the point has been investigated, it has been found that in comparing the results of the examination of the blood of those affected with elephantiasis and those not so affected, the frequency with which the blood of the latter contains the filaria is far in excess of what is the case as regards the blood of the subjects of elephantiasis. So much so, that if in a filarial district I wished to procure in a hurry specimens of the filaria, I would reject for my examination all cases of elephantiasis. Still, this marked rarity of filaria in elephantiasis I regard as an argument in favour of, though not as a proof, that the disease is correlated to, and probably caused by, the filaria—a negative relationship, if I may so express it, existing between disease and parasite. It almost looks as if elephantiasis were a sign of acquired immunity from the filaria.

The Pacific Islands, as a field for epidemiological and pathological study, offer unique but disappearing opportunities. For example, in many of them elephantiasis occurs in 20 per cent., even 50 per cent., of the population, and in some of them filariasis occurs in some 30 per cent. to 60 per cent. of the population. There are small islands with a population of 200 and 300, on which it is said

there is no elephantiasis. There are others equally small, in which nearly every individual, including Europeans, is affected. Under such circumstances, by systematic examination of the blood of the entire population, it would be an easy and very simple matter to decide as to whether elephantiasis is or is not a filarial disease.

Another point in connection with elephantiasis could also be cleared up. Lymphangitis of an acute character is practically always a forerunner and concomitant of elephantiasis. What is the immediate cause of this inflammation? I have sought to explain its nature and origin by a bacterial infection falling on an area of lymph stasis, the latter being primary and produced by the filaria, the former being secondary and produced by accidental inoculation of some pathogenic bacterium through a wound or insect bite. This is not an unimportant point to determine as regards Europeans. I have seen a good many missionaries who have lived in these islands, and who have become affected with elephantiasis of leg or arm, and I have heard of a good many more. In none of these missionaries whom I have examined have I found the filaria. Is it possible that we have a disease produced by some hitherto undiscovered organism? A disease consisting of a form of lymphangitis, which ends in the production of elephantiasis. A disease constantly agreeing in geographical range with the filaria, affecting the same set of vessels and the same tissues, but yet absolutely independent.

These are but a few of the problems with which such an investigation as I suggest might grapple. The practical importance of their solution is obvious, and I hope that ere long some attempt will be made to effect it. Lately, a good many pathological expeditions have left this country and the Continent for the investigation of plague, cholera, malaria, and other tropical diseases. Some of them have done excellent work, but much remains to be done. It will be strange if in a country which provides liberally for what might be called sentimental expeditions—such as the discovery of the North Pole, or for settling mere geological points of not very great practical importance, such as the determining of the process by which the Coral Islands acquired their peculiar conformation—it will be strange if in such a country funds cannot be provided to attempt, in the way I suggest or otherwise, the settlement of important questions affecting the life and comfort of men, and the material prosperity of the country.